NERVE POWER.

SECOND LECTURE OF DR. BROWN-SEQUARD. SOME OF THE FACTS THAT ARE DIFFICULT TO EXPLAIN—A NEGRO REDUCES CONVULSIONS BY PULLING AT THE GREAT TOE—MORE PERSISTENT VITALITY IN AMERICA THAN IN EUROPE, BOTH IN MEN AND ANIMALS—VARIOUS RELATIONS BETWEEN THE NERVOUS SYSTEM AND THE ACTION OF THE HEART—METHODS OF CHECKING CONVULSIVE EFFORTS, SUCH AS COUGHING, &C.

[FROM AN OCCASIONAL CORRESPONDENT OF THE TRIBUNE.]

BOSTON, March 1.—Dr. Brown-Séquard delivered his second lecture last night to a large audience:

LADIES AND GENTLEMEN: In the last lecture I tried to show several points relating to the force which we know to exist in nerves. I particularly insisted on what I call the unity of force in the nervous system. I especially tried to show that every nervous action is the cause of an expenditure of nervous force. There are a few facts, however, which may be considered as constituting an objection to that. I will mention some of them. The principal one is that we know full well that certain parts of the body may be extremely weak while others remain strong. But that certainly is no objection, since if we admit that the communication is obstructed between the part which is weakened and the rest, it is quite natural that there should be a diminution of force in that part. Besides this, there is something in the nervous system as well as in the muscles that permits a reaction after an irritation. There is a property of nervous tissue and muscles especially which we call excitability. The excitability of the nervous system is entirely and absolutely independent of the amount of nervous force. Perhaps it is wrong, however, to say as I have just done, that there is no dependence of one upon the other. There may be a dependence in this way, that the greater the amount of nervous force, the less excitability there is, and *vice versa*, the greater the amount of excitability the less amount of nervous force. People who have been ill; people who are naturally extremely weak, or those who have lost a good deal of blood and have been weakened in that way; in other words, people who have very little nerve force, are, as is well known, extremely excitable. They will jump at a noise and in other ways show nervousness. There is, therefore, something quite distinct in these two things—excitability and nerve force. This property of excitation is nothing but the power to receive an excitation. Persons who are extremely strong, will not generally be moved by excitation. They will, of course, appreciate the excitation; they will judge what it is; but they will remain calm under it. While, on the contrary, persons whose nervous system is weak, and who have little nerve force, will react under any excitation, however slight, without giving to the mind time to think of what the excitation is.

I said in my last lecture that the nervous force is quite different from electricity, that it is a force by itself; but I must add to this statement another one, that the nervous system is more or less charged with electricity all the time in health. The two forces, electricity and nerve power, are both present; but not always in proportion one to the other, as sometimes there may be an opposite condition. But certainly the nerve force is not electricity, as we well know that the speed of the nerve force is only from 80 to 300 feet in a second, while the speed of electricity as you know is thousands and thousands of times greater.

THE INFLUENCE THAT IS EXERTED UPON THE NERVES.

I now come to the principal object of this lecture, which introduces a subject that must extend to one or two more lectures—that is, the influence that the nervous system exerts upon itself by the force that we call nerve force. There are two kinds of such influence, which are absolutely distinct one from the other. One consists in the production of the activity, either normal or morbid; the other consists in the cessation of the morbid or normal activity. These two great influences of nerve force, acting upon parts at a distance more or less great, cover almost all the facts relating to the influence of the nervous system upon itself. I shall this evening enter more fully into the history of facts which show that the nervous system can stop the action of a part of its extent. All we know on this subject is of comparatively recent discovery, and the principal fact developed is that which relates to the heart. The brothers Webber discovered that the heart in a perfectly healthy man may be stopped suddenly in its action in a way which is quite different from that which regards muscles generally. If we galvanize a muscle that has been more or less in contraction by a current passing to and fro, stopping and passing again, so that the muscle is contracting and relaxing, as I show you now in this movement up and down exerted by the front of my arm—suppose that this is acting, and I pass a current into the nerves that goes to the muscles thus acting, immediately the movement stops; so that there is something similar to the cessation of the action of the heart. But the action stops, not because the muscles have stopped acting. On the contrary, the muscles are acting with a wonderful power, the greatest that they may have under the amount of electricity that is passing; and the contraction remains perfectly fixed so long as the current passes. This is the production of an active state in the muscle, and not the production of a passive state. According to the discovery of the brothers Webber, when the big nerve in the neck that goes to the heart, and which we call the par vagum, is thus influenced, the heart stops, passively, not actively, like the muscles of my arm. The walls of the heart remain perfectly flaccid, perfectly motionless from want of activity. During that time the heart is filled more and more by blood reaching it, and it becomes very much distended after a short time, as it does not reject the blood it is constantly receiving. There is, therefore, in that stoppage of the heart's action a phenomenon quite peculiar. And it is a phenomenon which implies a certain kind of activity. For although there is a passive effect obtained, a passivity produced in the heart, there is an activity in the nerves that go to the heart to produce that cessation. There is an influence upon certain parts of the heart belonging to the nervous system, and it is certainly an activity although it consists in stopping a movement. It is just as if you were to stop the wheel of a carriage by pushing a wedge under it forcibly.

ACTION OF THE NERVOUS SYSTEM ON THE HEART.

The great agents of the rhythmical movement of the heart are small ganglia, composed of cells of gray matter. They are suddenly rendered passive by the peculiar influence exerted upon them. Such an effect has been observed, first, by the galvanization of the nerve of the neck that I spoke of; and it was afterward found to appear when the medulla oblongata, or center from which that nerve starts, was galvanized. In experiments made by a French physiologist—Legallois—it was found that the crushing of the medulla oblongata produced an arrest of the heart. But he did not discriminate between that kind of cessation and death. He thought that death was caused by this crushing of the medulla oblongata, and that the heart had ceased because it had lost the source of its action. When I took up the question I found that a simple pricking of the medulla oblongata could produce an arrest of the heart's action. My friend, Prof. Charles Rouget, who took up the question of the mechanism of the phenomena by which an organ is arrested in its activity, considered that what takes place in the heart is similar to other phenomena which he noted at the time he published his paper. He established this law: that all such phenomena—which I shall call the phenomena of arrest, though in English they are generally called inhibitory phenomena—occurred always through the same mechanism. An irritation starts from a part which can convey nervous force, and the nervous force so conveyed after that irritation reaches the cells of gray matter which were active, and those cells of gray matter are immediately stopped by that peculiar influence.

For another illustration of this mechanism we are indebted to the observation of a very intelligent negro, whose master was affected with a disease of the spinal cord which produced convulsions in the lower limbs. The most intense stiffness would manifest itself in the lower limbs. They were rigid like a bar of iron for a time; and after ten minutes of this extreme rigidity they began to have violent jerks. The jerks then disappeared and the rigidity returned. All day long the lower limbs were in this state of muscular contraction. His servant, the negro, having to dress him, found it very difficult to put on his pantaloons. One day, he by chance took hold of his big toe, and found as he pulled it that the limbs became perfectly soft and movable. The convulsions had disappeared altogether. [The negro certainly had a natural genius for science. [Laughter.] He learned the meaning of the fact. He learned that whenever he wanted to push his master's pantaloons up, he had only to pull his big toe down. [Laughter and applause.] He succeeded every time. And as the master found the cessation of the convulsions useful at other times besides when he was dressing, the negro was asked very frequently to act on the big toe in order to effect it. [Laughter.] This fact is not a unique one. I have seen 14 such cases. Many of my medical friends have seen them also. In fact it seems somewhat a rule in cases where there is a certain disease of the spinal cord limited to a certain part, that this will be found. In this case you find exactly the same thing that exists in the heart when the *par vagum* is galvanized. In both instances there is a nerve that conveys irritation to the cells. In the case of the heart the nerve goes to the cells that are in the heart. In the case of the big toe, the nerve goes to the cells that were in a morbid state producing those convulsions. In the one case, that of the heart, the phenomena of movement were nominal; in the other, the phenomena were morbid. Still, it was the same mechanism in both. In both instances a cessation of activity was produced.

PRESSURE ON THE NECK TO CHECK THE HEART'S ACTION.

A friend of mine, Dr. Waller, a most intelligent man, a man of genius—although he was not a negro—found that by pressing on the neck he could produce the most interesting physiological phenomena. He has succeeded in that way in curing headaches, neuralgia of the face, and many other affections in which there was pain or great congestion of the head. An attack of epilepsy may be stopped in that way. Many physicians before him had produced some of those results, but they all thought it was from a pressure of the carotid artery. Dr. Waller has the merit of showing that it is chiefly—he thought it was only, but I have found that it is chiefly, not only—through an irritation of that nerve, the *par vagum*, that the motion of the heart is arrested in those cases, and that a diminution of the beating of the heart was followed by an amelioration in the circulation in the head, a cessation of an attack of epilepsy and of various other complaints. It was something, therefore, quite different from the mere pressure on the carotid artery. These views were not absolutely complete, as I have found that another nerve which goes to the blood vessels of the brain is also irritated by the process; and that the pressure exerted in the neck produces three effects: (1) It certainly diminishes the current in the carotid artery, and indeed stops that current altogether if the pressure is considerable; (2) it diminishes the circulation considerably, and may induce a profound state of syncope by acting on the *par vagum*; and (3) it also acts on the cervical sympathetic, and produces a contraction of the blood vessels in the head, by means of which a part of the good effect is obtained.

There are perhaps no parts of the nervous system which cannot under irritation have an influence on the heart to stop it. Even irritation of the nervous fibres of the brain may produce a cessation of the activity of the heart. Physicians in this room know perfectly well that sometimes a patient stricken down with apoplexy may have a great reduction in the action of the heart, and sometimes syncope may take place, resulting in death. In the spinal cord it is so also. There are indeed parts that cannot be pricked by the finest needle without some influence on the heart. Legallois, the French physiologist, of whom I have already spoken thought that all of the spinal cord was a center for the movements of the heart, and he had made experiments which seemed certainly to show that that was the case. He had passed a small bar of iron along a part of the spinal cord, and had found that that stopped the heart's action. But he made a mistake in considering that it was because he had destroyed the nerve center of the heart.

GREATER VITALITY IN AMERICA THAN IN EUROPE.

This experiment was the occasion, as perhaps some of you already know, of my finding that animals in this country can bear an injury far more easily than the same animals in Europe. I have ascertained that it is so for man also. And this is why so many medical writers in Europe consider that facts of this kind published here are mere inventions. There is a distrust among European physicians in the honesty and uprightness of American physicians, because the former cannot understand how man in this country can survive terrible injuries which would be fatal to him in Europe. I would not say that the truth is absolutely respected in this country or anywhere else, but still there is no doubt that the facts which have been mentioned are perfectly true. Experimenting on a rabbit before a class in the University of New-York, I had announced to them that pushing the instrument as I was about to do, along the cord, would be quite enough to kill the animal immediately. Fortunately for me, I had said that death was due to the hemorrhage accompanying the instrument, and not to the lack of the influence of the spinal cord. After pushing the instrument in for some distance, I found the rabbit which had been operated upon, eating a carrot [Laughter]. The class laughed more than you do now, and not at the rabbit but at me. [Laughter.] I could not understand at first what it was due to, and I then pushed the bar of iron its full length, or nearly one-half the extent of the spinal cord, but the rabbit continued to eat its carrot. Fortunately for me and for science, I found that there was no hemorrhage at all. I then took up the rabbit by its ears and showed that there was no bleeding, and explained in that way the persistence of life. What I had said, therefore, was verified by the fact that in Europe death takes place by hemorrhage. This tendency to hemorrhage in European animals is one of the differences between the animals of the two countries, and there are other important differences.

The heart can be stopped by a blow on the belly. Long ago Dr. Hunter had determined this. The fact was known before him, but he insisted on it. But the explanation was not what he thought. But it takes science a long while to move; progress is had slowly; and in this case it was a long time before the facts were generally received and a true explanation reached. Goltz, a physiologist in Germany, has made experiments on a frog, consisting in giving a blow of the finger on the belly; he has repeated the experiment which had been made so many times on man. Only since his time has it been known that the sympathetic nerve there has the power of stopping the heart's action. I had published different researches showing how it is that in peritonitis, which is an inflammation of the thin membrane in the abdomen, death occurs from lack of action in the heart. It is owing to an irritation of the ramifications of the sympathetic nerve in the abdomen, that the heart's action stops. This is very important to know, as if we possess the means—and we do possess them—of diminishing the irritation that takes place in these cases in the abdomen, we may save the life of the patient. It is well known that the means which frequently saves in peritonitis, that is the use of opium diminishes the excitability, and in that way prevents the influence on the heart. That influence goes up from the abdomen to the spinal cord, and rises there to the *medulla oblongata*, and then descends from the *medulla oblongata* by the *par vagum* to the heart.

A great many facts which you may observe at water-cure establishments, show the influence that cold possesses, by acting on the skin, in diminishing the action of the heart. There are a great many persons who are said to have no reaction after having been submitted to a cold douche or shower, and there are many who are in danger of dying from this treatment. Indeed, the person I know most intimately is absolutely unable to receive a douche of water without being in danger of dying from a cessation of the heart's action. In experiments made by two friends of mine, Dr. Dickinson and Dr. Bence Jones, they had pushed so far the influence of cold water on the skin that they had actually had an arrest of the heart's action. It shows, therefore, that there is danger in the douche or shower-bath, and that persons who have not the proper reaction ought not to continue to expose themselves to such a cause.

There are many other causes that may stop the heart's action. It is perfectly well known that emotion can do it. In all such cases it is by pretty much the same mechanism. Chloroform kills in that way. One or two breathings of chloroform may be sufficient, by the influence exerted on the ramification of the *par vagum* in the lungs, the irritation going up to the *medulla oblongata* and then down to the heart and arresting its action. This is only to be feared, however, in persons whose nervous system is very excitable.

In the larynx we also find what an effect may be produced on the action of the heart. I have ascertained that by putting carbonic acid in the larynxes of animals, the heart's action may be stopped immediately. Still I am bold enough in many instances to push carbonic acid with great violence toward the larynx, when it acts at the same time on the mucous membrane of the mouth, and loses something of its bad effect which consists in the arrest of the heart.

DANGEROUS METHODS OF CURING HEADACHES.

In one instance I found that a mode of curing headaches which is now employed may be liable to fatal results. A friend of mine had a very bad headache. I thought that if I could galvanize the cervical sympathetis in the neck, which goes to the blood-vessels of the head, I should produce a cessation of the pain almost at once. I succeeded admirably, but I almost succeeded in killing my friend. The heart's action stopped, and he was in great danger of death from a galvanization of the *par vagum* which had taken place at the same time I was galvanizing the sympathetis. Since that, I have been more prudent, and have not repeated the experiment. Many physicians, however, galvanize the sympathetis. They do it, it is true, in a way which is different from the one I employed; they apply the currents with more care. Still, I cannot but confess that there is danger in the process.

I pass now to what relates to the arrest of respiration. There is no doubt that the respiratory movements are all due to an activity of cells of gray matter, just as the movements of the heart are; the cells of gray matter, as regards respiration, being placed on the base of the brain and in a part of the spinal cord. The same nerve, the *par vagum*, which goes to the heart, has a set of fibers which, instead of going down, go upward, and toward those cells of gray matter in the base of the brain and spinal cord. So that if you divide the *par vagum*, having one hand by which you can act on the heart and another by which you can act on the brain, you can at will, at one movement, stop the heart's action, and in another stop the respiratory movements. The stopping of the respiratory movement is very peculiar. I have unfortunately no time to enter into details about it. But there are two kinds of nerve fibers able to stop the respiratory movements. There is one kind, according to Rosenthal, going to the larynx, acting by the nerve which is called the superior laryngeal. This stops respiration by the cessation of the diaphragm, which is the muscle that dilates the chest. This is rendered soft and inactive first as the heart is rendered soft and inactive by the galvanization of the nerve. The other part of the par vagum stops respiration by another mechanism quite different, which I shall not stop to describe.

But respiration can be stopped by a great many other means which are important to be known. It is important to know, for instance, that by passing a current of carbonic acid through the larynx, we can diminish the activity of the respiratory movements almost at once. I have seen convulsions stopped immediately by the passage of carbonic acid in that way, and the respiratory movements themselves may be stopped altogether for a time; and as you are sure that they will return if you stop acting with the carbonic acid, you have there a means of diminishing the influence of a morbid state of respiration.

There are facts which I should have mentioned regarding the heart, which relate also to respiration. If we take a pair of bellows and insufflate air into the mouth of an animal, we find that the activity of the heart is diminished. If we do the same with a view of affecting the respiration, we find that the animal does not then take the trouble to breathe. It seemed to the physiologist that first made the experiment that, as he was giving the animals all the air they needed, they would be perfectly stupid to take the trouble to breathe. [Laughter.] The reality is that they do not think at all about it. I may say that they have no power of thinking, as in many cases the activity of the mind is lost for the time. But even if the mind remains, there is a cessation of the activity of the cells that serve respiration by the irritation of nerve-fibers in the bronchia. I have ascertained for instance that if you divide the par vagum in the neck so that the communication between the bronchia and the brain no longer exists, if you insufflate carbonic acid into the lungs there is no more stoppage of the activity. Therefore the stoppage took place through the influence that was propagated in the ramifications of the par vagum toward the brain. As Hering has insisted upon, there are many facts which show that the very effort of breathing brings with it a cause that stops breathing. The very fact of drawing in air is a cause which stops the action of drawing in air. He has gone a little farther than I should go in saying that the expulsion of air from the lungs is also a cause of stoppage of expiration. It seems in reality as if these three movements, the movements of the heart, of inspiration, and expiration, had associated with them a cause that diminished them. When that cause is deficient, in morbid states, then we find the movements of the heart becoming exceedingly rapid, and we find the movements of respiration becoming exceedingly rapid and tumultuous. The regulation of those movements belongs to the proper action of those powers of arrest which exist there. As regards the heart, in cases of palpitation, for instance, we have a simple means of diminishing the palpitation; it is breathing in rapidly and forcibly a good deal of air, dilating the chest as powerfully and quickly as we can. In that way an influence is developed which I have found to be the result of the association of the nerve force that goes to the muscles of the chest and the force which descends and stops the heart's action. At the same time that the current goes from the brain to the muscles of the chest to dilate it a current associated with that goes down the par vagum toward the heart to diminish its action. In health at every moment this thing takes place. It takes place in a very slight degree indeed. Every act of breathing is an act which moderates the action of the heart. So then there is an admirable provision of nature by which an excessive action finds a moderation in something which takes place usually along with it.

MEANS OF CHECKING COUGHING, SNEEZING, &C.

There are many facts which show that morbid phenomena of respiration can be also stopped by the influence of arrest. Coughing, for instance, can be stopped by pressing on the nerves on the lip in the neighborhood of the nose. A pressure there may prevent a cough when it is beginning. Sneezing may be stopped by the same mechanism. Pressing also in the neighborhood of the ear, right in front of the ear, may stop coughing. It is so also of hiccough, but much less so than for sneezing or coughing. Pressing very hard on the top of the mouth inside is also a means of stopping coughing. And I may say that the will has immense power there. There was a French soldier who used to say, whenever he entered the wards of his hospital, "The first patient who coughs here will be deprived of food to-day." It was exceedingly rare that a patient coughed then.

There are many other affections associated with breathing which can be stopped by the same mechanism that stops the heart's action. In spasm of the glottis, which is a terrible thing in children, as you well know, as it sometimes causes death, and also in whooping-cough, it is possible to afford relief by throwing cold water on the face, or by tickling the soles of the feet, which produces laughter and at the same time goes to the gray matter that is producing the spasm and arrests it almost at once. I would not say that these means are always successful. I would not say that we can always prevent cough by our will; but in many instances those things are possible, and if you remember that in bronchitis and pneumonia, or any other acute affection of the lungs, hacking or coughing greatly increases the trouble at times, you can easily see how important it is for the patient to try to avoid coughing as best he can.

There is also a series of other convulsive movements more or less associated with breathing, and it is very important in those cases to counteract the influence by action on certain parts. There is a form of epilepsy which consists almost exclusively in what Basil Hall has called laryngismus. He had an idea that it was essential to open the trachea and let the patient breathe through an opening there. But this is not at all necessary, even if it did good. Touching the larynx with a sponge charged with a solution of nitrate of silver will very frequently prevent laryngismus, when it has just begun and it has very little power. But in those cases of laryngeal epilepsy, in which the convulsions come from affections caused by a spasm of the larynx, there is no doubt that this device or expedient changes the activity in the muscles, and that activity is enough to produce a cure.

There are a good many other phenomena of arrest. The most interesting are those relating to the brain. I cannot in this lecture speak of more than one of them, and that is arrest of the cerebral activity, of thought, of consciousness. It is well known that in epilepsy cerebral activity is lost. It is well-known, also, that in certain cases of syncope it is lost. In cases of sleep, also, it is lost, except of course in great dreamers, and then there is hardly any consciousness, and in any case the condition is quite different from that of wakefulness. There is an evidence that a theory which I advanced long ago to explain the loss of activity in the brain is only partially true. It was that a contraction or spasm takes place in the blood vessels of the brain, that blood does not circulate there any more, and that, as I then supposed, the stoppage of the circulation causes a cessation of the activity of the brain. But there is another cause in these cases in which there cannot be a contraction of the blood vessels, because the principal nerve which produces these contractions has been divided; and even in those cases a loss of consciousness can take place suddenly. Pricking the base of the brain may cause a complete loss of consciousness in an animal after a division of the nerves that go to the base of the brain. We cannot attribute that loss of consciousness to want of circulation, for the blood may be circulating there. But there is another fact. If we galvanize the par vagum so as to arrest the heart's action, there then is no circulation at all in the brain; and if we have galvanized only the part of the nerve which goes to the heart after having divided it, so that there is no circulation at all in the brain, the animal remains conscious for eight or ten seconds. Therefore, when we find in some cases that certain irritation will produce a loss of consciousness immediately, we cannot look upon the loss of consciousness as being due to the cessation of circulation in the brain, as that cessation does not immediately cause the loss of consciousness. In cholera, too, the brain remains active sometimes, although there is no circulation in the brain, although the blood there is perfectly black. It shows that the brain may remain active when deprived of circulation, and even of oxygen. We must therefore admit that there is an active cause which produces the cessation of the activity of the brain in those cases in which it is produced by the cessation of the nervous system.

A BOND BASIS BUT NO INFLATION.

To the Editor of The Tribune.

SIR: Mr. Senator Morrill of Maine complains that his brain is "muddled" by the discussions going on around him on the financial question. Mr. Morrill should read THE TRIBUNE and get clear of his perplexities. There is nothing bewildering in the financial problem to anybody who will look at it steadily from the correct point of view.

There are just two points to be considered, and two only; and there are just two parties to the question, and two only. One of these parties desires contraction and a return to specie payments. These two things we put together, because we consider them to be identical. The other party desires expansion by the issue of more paper money. Our plan is to satisfy both parties to a reasonable degree.

Instead of redeeming our paper money in specie, we propose its redemption in a 5 per cent gold bond of the United States. This is something valuable and valid. It is a return to something like coin payments. For such a bond has a commercial value in our own and all the markets of the world at a steady rate, approximating the value of coin. Why should not hard money men, and redemptionists of any degree, unite in the support of such a measure? It is a most important step in a conservative direction, and will bring us nearer, much nearer, to specie payments than we now are.

We say to the inflationists, accept this reasonable basis of redemption and we will go with you to break up the existing monopoly of banking, and throw it wide open to everybody everywhere. No community that desires more banks, no town or village that desires a new bank, shall be denied.

Here, then, are two propositions of the plainest and most practical character, that need "muddle" no man's brain; which embrace the whole subject, or so much of it as Congress is now called to act upon. Both are before the Senate, and have been voted on several times during the past week, and both have received the votes of nearly a majority of that body. We cannot help thinking that the majority of Congress, Mr. Senator Morrill of Maine included, must, sooner or later, get rid of their bewilderment, and perceive in these simple propositions the true solution of the financial question in its present phase.

It is entirely certain that the extreme measure of resorting to the resumption of coin payments will not now be adopted, either by boldly facing the issue or by the acceptance of any of the sugar-coated measures proposed by one and another of our financial reformers. And we suppose it to be equally true that Congress will not pass the ruinous and unconstitutional measure of issuing more greenbacks. Our practical danger is that we shall get inveigled into a wild-cat banking system; that is, of permitting the establishment of banks of issue, without holding them to any plan of redemption except, as now, in irredeemable notes of the Government. If we go to expanding our circulation in that way, we shall bring on another crisis, which Congress will be called on to relieve by widening the basis of banking, by the issuing of more greenbacks. If we once get fairly embarked in this see-saw business, our whole paper money system will be thrown into confusion and ultimately explode.

It will be but little better if Congress attempts to control expansion by limiting the amount of the aggregate issues of the banks. This is a dangerous reliance; for every time there is a pressure, an excuse will be found to enlarge the sum.

The only safe way, and the way that can be least complained of, is to leave the whole banking field open for anybody to work who will, but holding all who issue paper money, Government included, to a redemption of their issues in something specific and commercially valuable. This, and this alone, will prevent dangerous inflation. J. S. F.

Philadelphia, Feb. 23, 1874.

THE POLARIS VOYAGE.

NEW DETAILS BROUGHT OUT BY THE INVESTIGATION.

DIFFICULTIES WHICH PREVENTED FURTHER APPROACH TO THE POLE—THE SCIENTIFIC RESULTS OBTAINED AND THEIR ACTUAL VALUE.

The complete accounts of the Polaris Investigation give many interesting particulars which were not supplied in the telegraphic dispatches at the time. From the report now issuing from the Government Printing-Office we make the following extracts:

Capt. Buddington's testimony is very definite as to the impracticability of pushing the vessel further north than the point which they reached. He says:

In the morning, about 4 o'clock, we came to a full stop, and Capt. Hall told me to go in on the east side. There was no chance to get westward or any way, except into the east from where we were, and he said we would look for a harbor. I accordingly steered square in across the channel for the east side and ran alongside of one heavy floe about six miles, by Walker's patent log, and in the evening the captain tried to land with Capt. Tyson; but the tide was running so fast he could not get ashore without losing his boat, on account of the ice. That night we had it foggy. That was at what is called Repulse Harbor, above Newman's Bay. The next morning he tried to land again. We knocked about that night and took every advantage to get north that we best could, but did not succeed. It was foggy during the night—somewhat misty. Next morning Capt. Hall landed again with Mr. Tyson—or rather tried to land at the same spot, but they could n't get ashore. They come near losing their boat, as was reported to me. It was all I could do to keep the shore there, the heavy floe was going so fast. There was a little water inside, but it was a very strong tide. It was about the full of the moon, I think: I do not recollect now, exactly; but I know there was a very strong tide running to the south. We had the ship under steam, and when Capt. Hall came aboard he took a look at it, and I recommended strongly that we should go to Newman's Bay, which would be an open place. That was about eight or ten miles south of us. That was the only place I could see, and I thought it best to go in there, and if the channel cleared we could see it and be open to it, and not run the risk of getting beset in the ice in trying to stay out there. He held a council with the officers—Dr. Bessels and myself, and the others—which I have here, that was written down as it occurred, I believe, word for word. It read as follows:

"Consultation held on board the Polaris in regard to getting further north with the vessel, the vessel being on the east side looking for a harbor. Dr. Bessels, Mr. Meyers, Capt. Tyson, Capt. Buddington, Mr. Morton, and Mr. Chester. Doctor wanted to cross the straits to look for a harbor, as being better for sledge journeys, while the east side was better for navigation, if we could not get further north. Mr. Morton coincided with Dr. Bessels; Mr. Meyers had the same opinion; Mr. Chester to get as far north as possible; Capt. Tyson to get into harbor as soon as possible; Capt. Buddington to keep on east side as being better for navigation, and certainly better for sledge journeys. It was impossible to get further north on account of the pack. Go along the coast on the east side of the straits southward until a harbor is reached, which could be done in a short time. There had been seen one a few miles to the south of present position of the vessel. It was decided by the commander to cross the straits. In doing so we got beset by the pack and drifted back about fifty miles."

Question. Was there ever any chance to get North with the ship after she got beset in Robeson Channel? Answer. No, Sir; none that I know of.

Question. Was there a consultation there by Capt. Hall with Chester, Tyson, and yourself? Answer. Not with me; I never heard of any.

Question. With anybody? Answer. No, Sir; not that I know of. There was something said after we got into Polaris Bay about the chance to get North. Capt. Hall stepped up to the hill himself and looked at the ice, and came back and decided that it was impossible. He never asked me about going further at all, but told me that to end the thing he would make that his Winter quarters.

The first mate of the Polaris, Hubbard C. Chester, is not so definite upon this point; in fact he throws a doubt upon it, as follows:

Question. If you had had command of that ship, could you have gone further north? Answer. I do not like to say anything of that kind. I should have tried hard to. I thought I could see considerable open water at the north. We knew by the water-cloud that there was an open sea of water there. That was evidence, because we saw all the time we were in the channel, when it was clear, this dense white cloud to the north. We knew, after we got through this channel, that we would be going into a large bay or sea of some kind. The best chance I saw was at the time we steamed in after we got out of the ice, when we were beset there. I think we could have gone up through the channel on the east coast, because the wind was to the north-east, and all the ice there was in the channel—was in the middle and on the west shore. Whether I could have done it I can't say, but I should have tried it if I had had the privilege.

Dr. Bessels submitted a memorandum of the most important discoveries of the expedition, namely:

The results of the expedition may be summed up briefly as follows:

1. The Polaris reached 82° 16' N., a higher latitude than has been attained by any other ship;
2. The navigability of Kennedy Channel has been proved beyond a doubt;
3. Upwards of 700 miles of coast line have been discovered and surveyed;
4. The insularity of Greenland has been proved; and
5. Numerous observations have been made relating to astronomy, magnetism, force of gravity, ocean physics, meteorology, zoölogy, ethnology, botany, and geology, the records of which were kept in accordance with the instructions supplied by the National Academy, and some of the results of which we propose briefly to enumerate.

Great care was taken in determining a reliable meridian at Thank-God Harbor. Soon after entering Winter quarters an observatory was erected on the shore, 34 feet above mean sea level, and the transit instrument stationed there. The longitude of this station was determined by the observation of 300 lunar distances; a number of moon culminations; a great number of star transits; a number of star occultations; and a great number of altitudes of the sun on or near the prime vertical. Its latitude by the observation of a great number of circummeridian altitudes of the sun, and a number of altitudes of stars. All of these observations were lost, but a number of the results have been preserved which are sufficient to establish the position of this station. Besides the above mentioned observations, 20 sets of pendulum experiments were made, which are saved, but the observations for time belonging to them are lost.

The magnetic observations obtained were more complete than any others ever before made in the Arctic regions. The instruments supplied were: One unifilar declinometer; one dip circle, with Lloyd's needles; one theodolite, and several prismatic compasses. The observations on variation of declination were registered at Göttingen time, and were continued for five months. Readings taken hourly. Beside that, three term days were observed every month, according to the Göttingen regulations, one of these term days corresponding with the day accepted by all the magnetic stations. Further, a number of observations were taken either with the theodolite or the prismatic compass. Whenever possible, the dip was observed, and several sets of observations on relative and absolute intensity and of the moment of inertia were obtained.

Unfortunately there was not much opportunity for taking soundings. About 12 were obtained along the coast of Grinnell Land, which prove that the hundred-fathom line follows the coast at a distance of about 15 miles in Smith Sound. One of these soundings (90 fathoms) proved highly interesting, containing an organism of lower type than the *Bathybius* discovered by the English dredging expedition. It was named *Protobathybius robesonii*. A number of deep-sea temperatures were taken with corresponding observations on the density of the water. Following the coast of West Greenland the limits of the Gulf Stream were ascertained. Specimens of water from different depths were preserved in bottles, but, unfortunately, lost.

After having entered Winter quarters meteorological observations, which up to this time had been made three-hourly, were made every hour, Washington time. The register contained observations on the temperature of the air, atmospheric pressure, psychrometrical observations, directions and force of wind, appearance of the sky, state of weather, and both solar and terrestrial radiation. Besides, all extraordinary meteorological phenomena were carefully noted.

Special attention was devoted to the aurora borealis, which occurred frequently, but rarely showed brilliant colors, never bright enough to produce a spectrum. Whenever necessary, one observer was stationed at the magnetometer and the other out doors, the former observing the motions of the magnet, while the other was watching the changes in the phenomenon and taking sketches. Although an electroscope and electrometer were set up, and the electrical condition of the atmosphere frequently tested, in no instance could the least amount of electricity be detected. The amount of precipitation was measured as carefully as the violent gales would permit, by means of a rain gauge supplied with a funnel. In February, as soon as the sun reappeared, observations on solar radiation were commenced and continued throughout the entire Summer.

The collections of natural history are nearly entirely lost. With the exception of two small cases containing animals, minerals, and one package of plants, nothing could be rescued. The character of the fauna is North American, as indicated by the occurrence of the lemming and the musk-ox. Nine species of mammals were found, four of which are seals. The birds are represented by 21 species. The number of species of insects is about 15, viz.: one beetle, four butterflies, six diptera, one bumble-bee, and several ichneumons, parasites in caterpillars. Further, two species of spiders and several mites were found. The animals of lower grade are not yet ready for examination.

The flora is richer than could be expected, as not less than 17 phanerogamic plants were collected, beside three mosses, three lichens, and five fresh water algæ.

Although the formation of the Upper Silurian limestone, which seems to constitute the whole west coast north of Humboldt Glacier, is very uniform, some highly interesting and important observations have been made. It was found that the land is rising, as indicated, for instance, by the occurrence of marine animals in a fresh-water lake more than 30 feet above the sea-level and far out of reach of the Spring tides. Wherever the locality was favorable the land is covered by drift, sometimes containing very characteristic lithological specimens, the identification of which with rocks in South Greenland was a very easily accomplished task. For instance, garnets of unusually large size were found in latitude 81° 30', having marked mineralogical characteristics by which the identity with some garnets from Fiskennes was established. Drawing a conclusion from such observations it became evident that the main line of the drift, indicating the direction of its motion, runs from south to north.

It would lead too far to enter into detail with regard to numerous miscellaneous observations that were made beside those mentioned above.

GRIEVANCES OF A POLARIS SURVIVOR.

To the Editor of The Tribune.

SIR: The reception of the officers and crew of the Polaris by the Geographical Society in Cooper Institute on the 16th inst., so far as concerns holding the attention of perhaps the largest audience that ever assembled within the walls of the building, was a success. But to the comparatively few who were particularly interested, and who understood both the lecturer and his subject, as also the remarks of Capt. Tyson and the motive which prompted him to make the assertion he did, it was only a source of mortification and sorrow, and I might add insult, did we not know too well "the most distinguished of living Arctic explorers" for his words to have affected us in any other way than to appeal to our sense of the ridiculous.

The most serious reflections were made on the good name, preëminent qualifications, and distinguished abilities as an Arctic explorer of our revered commander, the late Capt. Hall. Every member of the crew, except, perhaps, Mr. Tyson, felt shocked at the discourteous language and bearing of Dr. Hayes. He assumed, at times, to have us on exhibition, then in the witness-box, and finally he totally ignored every word ever spoken by any of us in regard to the non-existence of his mythical "open Polar Sea." He confidently asserted it as his conviction that the distribution of land and water was not as we had reported from actual observation, but as he had imagined. He yet claims to have "stood upon the shores of the Great Polar Basin," his eye reaching far out over the apparently "illimitable ocean which rolled in grandeur at his feet." We found this basin to be merely a slight expansion of the channel 35 or 40 miles wide above Cape Leiber, forming, on the opposite side, what we explored and called Polaris Bay. And it remained almost continually filled with ice during our entire stay of 11 months and 12 days. His reputed discoveries proved only a trap to Capt. Hall.

He took advantage of the large gathering of people—who had come not to hear him, but to hear of the Polaris by some one of those who had accompanied the vessel—to inflict on them his cut-and-dried lecture of the history, objects, &c., of Arctic discovery. Many can attest that this was the sixth or seventh time he has delivered it. After giving the audience a full explanation of this "great sea, equal in extent to the North Atlantic and open because of its great width and depth," and displaying on canvas his own ideal geographical composition, with an imaginary track of the Polaris until she had passed that formidable barrier, the ice-belt, which had hitherto excluded less fortunate vessels from the thermal waters of the Pole; piloting her with his magic wand safely through all this, on canvas, and meeting scarcely an obstacle, he leaves her, in imagination, calmly floating on the placid surface of this virgin sea. Carrying the audience with him in this remarkable flight, and disclosing to their mental vision the brave little ship with all danger left behind, and ready to proceed direct to her goal, he suddenly turned on us, and actually challenged the Polaris crew to inform the audience why we did not proceed northward. Apparently he now felt that he was about to annihilate us; regarding Capt. Buddington with a stare expressive of his conviction.

Having extended the "few minutes" in which he was going to detain the audience into an hour and a quarter, and smoothing the road for Capt. Buddington as above indicated, and having propounded the momentous question the answer to which was to forever settle our claims to an unfaltering discharge of duty, he took his seat. Capt. Buddington arose and modestly and truthfully stated in a few simple words the condition of the channel at, and above, as far as could be seen, the greatest point of our northing. But before the captain was seated, "the most distinguished of living" authors of Arctic romance had begun a reply, substantially that it was his opinion that the only difference between Robeson Channel, as shown by our surveys, and the "open Polar Sea," as described by him, was that of the impressions that would be made upon two parties observing the same appearances of a country. Such a proposition virtually arraigned the veracity of the whole crew. He intimated that if he instead of Capt. Hall had been in command the "Stars and Stripes would have floated over the very axis of the earth."

But from this unparalleled attack on the noble dead, and these insinuations on the living, he was partly exonerated by the utterances of G. E. Tyson, who now, for the first time since his connection with the expedition, intimated that Capt. Hall did not do all it was possible for any one to do in trying to work the vessel further north. He proceeded to inform the audience that there were unmistakable evidences of open water to the north, and that at one time he thought he saw it, supposing it be about eight miles distant; and that it was his conviction that by a persevering continuous effort we might have reached the water and have gone much "nearer if not entirely to the Pole." What interest Capt. Tyson had in making such a statement it is impossible to conjecture. The excitement of the occasion may have led him into such exaggeration.

Mr. Bryan was invited to give his views in regard to the particular question under consideration. Even under this embarrassing restraint, Mr. Byran thought it his duty to say a few words. He gave it as his conviction that no vessel, under the same circumstances, could have gone further north than did the Polaris. And in this position he was sustained by every member of the expedition except Capt. Tyson. It was a source of regret to all of us that Mr. Bryan did not speak longer and expose, as he well could have done, the pretentious fallacies that had been uttered. It was our desire to acknowledge through Mr. Bryan our thanks to the Society for this expression of their kindness and respect, and to entertain them with some important general features and a few interesting details of the discoveries of the Polaris. But the audience had been held till a late hour.

I was somewhat wrought up by these considerations, and seeing what I had in the past, and comparing our surveys with Dr. Hayes's glowing description of the country and the non-existence of North Greenland as asserted by him, I was anxious to say a few words myself, and inform the public of the manner in which they were being duped, had Judge Daly remembered to have called upon me. But, since no opportunity then offered, it has occurred to me that a letter published in the columns of your paper would answer the purpose equally well. I would have told how, during the Winter of 1871-72, when the Polaris was in Winter quarters closely locked in by the crushing ice which filled the channel from shore to shore (where even to-day Dr. Hayes represents the existence of a "great sea"), and when no sunlight illuminated the heavens, I was accustomed to look westward, and could clearly discern the outlines of the cape from which Dr. Hayes claims to have seen the "open Polar Sea." On Christmas morning, 1871, when the sun was nearly 15° below the horizon, even at noon, Dr. Bessels and I enjoyed a fine view of the opposite coast by moonlight where Dr. Hayes could not see across in the full glare of the long Arctic day. At that time we thought, how much better it would have been for him, if he had stated the simple truth. We thought then that if we were so fortunate as to regain the civilized part of the world, that his dissertation would be consigned to oblivion.

Mr. Morton was with Dr. Hayes on the Grinnell expedition commanded by Dr. Kane, and it is well known to us why this pioneer of Arctic travel was not present at this reception. I trust that this will help to lead your intelligent readers to examine the subject for themselves, and I doubt not that then they will do full justice to the memory of Capt. Hall, and also to the name and fame of his merciless detractors.

N. HAYES, seaman on the late Polar expedition.
Washington, Feb. 21, 1874.

READING FOR THE HOSPITALS.

To the Editor of The Tribune.

SIR: Allow me to state in reply to your correspondent who asked, "Where are the boxes for papers for hospitals?" that (through the kindness of the H. R. R. R. Superintendent) a large box was placed some weeks ago in the Thirtieth-st. Depot, and that since the date of his letter, but prior to its publication, a similar box has been placed by the exit door of the Grand Central Depot. The papers received in these boxes have been distributed daily among the patients in Bellevue Hospital for some time past.

As your correspondent takes an interest in the subject, will he not ask his fellow-passengers using these depots to throw their papers in these boxes; the present supply is sufficient only for Bellevue Hospital, while the boxes are large enough to receive the quantity requisite for the Charity Hospital, Hospital for Convalescents, Hospital for Incurables, and for the Soldiers' Retreat. If the passengers will drop their papers in these boxes, the State Charities Aid Association will have them distributed daily as far as they "will go round," but at least four times the present supply is necessary to supply all the hospitals. Truly yours,

ALFRED PELL, Secretary of Committee on Books and Papers.
March 2, 1874.

QUESTIONS BETWEEN THE PARK COMMISSION AND THE DEPARTMENT OF PUBLIC WORKS.

A CARD FROM COMMISSIONER VAN NORT.

To the Editor of The Tribune.

SIR: Permit me, by the statement of a few facts in relation to the scope and intent of the bill to authorize the prosecution of the improvement of the Riverside and other avenues, as reported by the Senate Committee on Cities, to correct some serious errors which occur in an editorial in to-day's TRIBUNE. Referring to this bill, the article says:

It substantially provides that the work on streets through and around the Riverside and other parks in that region shall not remain under the control of the Department of Public Parks, to be let by public contract to the lowest responsible bidder, but shall be managed by the Commissioner of Public Works as he pleases.

On the authority of the legal opinion of the Counsel to the Corporation, and of a message of his Honor the Mayor to the Common Council, vetoing an ordinance directing the improvement of the Riverside and Morningside-aves., it may be stated that the existing laws in relation to the jurisdiction over these avenues are so obscure and ambiguous that neither the Common Council nor the Park Department nor the Department of Public Works is clearly authorized to order or undertake the prosecution of these works, and both of these officials suggested the propriety and necessity of further legislation on this subject by the Legislature, which was then about to convene. The proposed law does not, therefore, take away from the Park Department any powers which it now possesses, but merely reaffirms the authority conferred on the Department of Public Works by Chapter 872 of the Laws of 1872, and which, in my opinion, was only partially abrogated by the amended charter, by giving the Common Council the power to pass ordinances directing the Department of Public Works to execute the work. Furthermore, during the whole history of the Central Park Commission and the Department of Public Parks no work on streets or avenues bordering upon parks was ever done by either of them, and such street improvements as were carried on by them under special laws—the Boulevard, the Sixth and Seventh-aves., and Avenue St. Nicholas—were invariably done by day's work or by special contract, and not by contract publicly let to the lowest bidder.

Your article further says:

Counting the avenues and roads established, or referred to, in the bill, there are over 12 miles of streets and avenues herein bodily made over to the Commissioner of Public Works—six miles around the Central Park, three miles around the Riverside Park, half a mile around Manhattan Square, three quarters of a mile around Mt. Morris Square, one and three quarter miles around Morningside Park, and half a mile around High Bridge Park.

This statement is greatly exaggerated. The six miles of streets and avenues around the Central Park, with the exception of One-hundred-and-tenth-st. from Fifth to Eighth-aves., and Fifth-ave. from Ninety-sixth to One-hundred-and-tenth-sts., are already regulated and graded; so are the streets and avenues around Manhattan Square and Mt. Morris Square; One-hundred-and-tenth-st. is now being improved under the Eastern Boulevard act. Under chapter 872 of the laws of 1872, above referred to, this Department commenced work upon the Morningside and Riverside-aves. in June, 1872, and prosecuted it until the passage of the charter, which, as already stated, relegated these works to the control of the Common Council. At that time two-thirds of the regulating and grading of the Morningside-ave. had been done, and considerable progress had been made on Riverside-ave. at the different points. The work to be done under the proposed bill is, therefore, not one third as large as would appear from your article.

In view of the fact that this Department ever since its existence, and its predecessor, the Street Department, before it, have been charged by each succeeding charter with the management and control of all the streets, avenues and roads in the city, which at this time measure 448 miles in length, there seems to be no ground for apprehension that it will be overpowered or crushed by the responsibility which the proposed bill intends to impose upon it. As to the opinion of the tax-payers and property-owners of the West Side, who are most deeply interested in these improvements, and upon whom the cost will ultimately be assessed, whether this Department possesses the will and the ability to carry on those works in a vigorous and economical manner, as well as with regard to their wishes for a speedy prosecution of the improvements, I beg to refer to the address of the President of the West Side Association to his Honor the Mayor, which appears in full in one of this morning's papers. Yours very respectfully,

GEO. M. VAN NORT, Commissioner of Public Works.
Department of Public Works, March 2, 1874.

[We gladly give space to Commissioner Van Nort's explanations, and only wish they might have removed the most serious objections to the measure now before the Senate, which we still think remain untouched.

1. We have not concerned ourselves with the question whether the Park Department now clearly possesses powers which this bill makes over to the Commissioner of Public Works. We have said that, in the interest of a single and not divided responsibility, we believed it ought to possess them.

2. We have not said how much, or how little, work was to be done on the various avenues and roads established or referred to in the bill. We have only said that they are herein made over bodily to the Commissioner of Public Works, whereas we believe they ought to be under the control of the Department of Parks.

The Commissioner of Public Works certainly has all the work now legitimately within his control which any one public officer need care to undertake; and we are glad to bear testimony to the fact that complaints about the manner in which it is discharged are comparatively infrequent. But we believe in having a single, not divided responsibility; and for the work in and about the Parks, we should like to be able to hold the Park Commission, and the Park Commission exclusively, responsible.—*Ed.*]

LAFAYETTE COLLEGE AND THE LITERARY CONTEST.

To the Editor of The Tribune.

SIR: Allow me, through the columns of your journal, to make a correction in the report of the proceedings of the Intercollegiate Convention held at Hartford Feb. 19, so far as it relates to Lafayette College and the Lafayette delegates. I find in the report, as published in THE TRIBUNE of the 20th inst., and as published in *The Yale Courant* of a late issue, the following:

"Lafayette delegates were sent by the Faculty, but were bound by the Convention, provided the contest does not take place at Saratoga."

This conveys the impression that the Faculty of Lafayette College are favorable to the project of intercollegiate contests, that they are now bound by the action of the Convention inasmuch as the proposed contest is to take place in New-York City, and that they are and were opposed to holding the contest at Saratoga, and so instructed the delegates to vote. So far from this being the truth, we may say that the Faculty of Lafayette did not send the delegates, but only permitted them to go when sent by the students; that they did not commit themselves in the least with reference to the Convention or the contest, and never even mentioned Saratoga or any other place for holding the contest, favorably or otherwise. The judgment of the Faculty was that contests are not feasible, and that the Convention, if it did anything, would not adopt any generally satisfactory plans. How such an impression as that set forth in the report could have taken hold of the Convention or the reporters, it is impossible to understand, for the delegates from Lafayette in their statements at the Convention made use of no such language. They did say, however, that they were sent by the students of Lafayette under leave of the Faculty, that they were without instructions or power to act for their constituents, but only came to the Convention to see whether it meant business, and to report that business, if any should be transacted, back to their constituents, who were indifferent as to whether the Convention should mature plans for a contest, and doubtful as to whether any plans would be feasible. No mention was made of Saratoga whatever, and the delegates favored New-York City only as being a literary center, and as likely to harmonize the discordant elements of the Convention. No action has yet been taken to ratify the provisional constitution adopted at the Convention.

A DELEGATE.
Lafayette College, Easton, Penn., Feb. 28, 1874.

THE KING JURY STILL INCOMPLETE.

The work of testing jurors for competency to serve on the trial of James C. King, indicted for the murder of Anthony F. O'Neill, was resumed yesterday, before Judge Brady in the Court of Oyer and Terminer. District-Attorney Phelps and Assistant District-Attorney Lyon represented the people, and Messrs. Beach, Mott, and Howe the defense. About half the number summoned in the panel of 102 answered to their names. James Gullen, who had derived an impression of horror from reading of the homicide; Wm. M. Rogers, who had scruples against capital punishment; Ferdinand Kerns, who had a prejudice against the defense of insanity; James Burchett, who did not believe in emotional insanity; and David Klein, who had scruples against hanging a man, were excused. James Haydock, who believed that people were too often made out to be insane when they were perfectly sane, was also excused. Francis Darr, a soapmaker, of No. 22 Park-ave., and Frederick Link, of No. 312 West Thirty-fourth-st., were sworn as the ninth and tenth jurors, and the rest were set aside for previously-formed opinions as to the prisoner's guilt or innocence. The panel being exhausted, the Court adjourned to this morning.